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Sir James Gowans CBE FRS in interview with Dr Max Blythe
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Interview Five

MB Sir James, it's 1998, the centenary of Howard Florey's birth. We are here in Oxford and it's good talking to you because you spent quite a long time working with Howard Florey in Oxford at the William Dunn School of Pathology. Where did you first meet Howard Florey? I think it was before then.

JG Yes. I didn't actually meet him, but I certainly, as it were, sat at his feet because when I was a medical student in London, at King's College in the Strand, doing pre-clinical work, I used to go to the Friday evening discourses at the Royal Institution, and Florey who was famous by that time for the penicillin work...

MB 1944?

JG Yes. He gave one of the Friday evening discourses about penicillin, which caused enormous interest, with the amphitheatre at the Royal Institute absolutely packed to the ceiling, you know, for this lecture. I had never seen Florey before. I'd seen pictures of him because he was a famous man. But I must say he gave a very good lecture with nice demonstrations. I can remember one - they were very simple demonstrations but quite graphic - demonstrating the power of penicillin by having an enormous urn of fluid and dropping a few grains of something in to show the concentration of penicillin which was bactericidal for a particular bacterium, and so on. I mean, they were very simple but graphic. And [culture] plates and Norman Heatley's very nice cylinder plate assay, which you have no doubt seen. All this very simple, easily understandable science, which, in fact, concealed a really epic piece of work during wartime. As we said many times, translating a laboratory curiosity, which was Fleming's 1929 paper, into the most powerful therapeutic agent that had ever been used in medicine, and was now used in the war, you know, to treat war wounds. So there was this down-to-earth but very vivid, strong character, you could see that. One could get an impression of somebody that ran things around here, you know, describing in very simple terms this sequence of events that led from a bit of bacteriology to something in a phial which was curing war wounds, you know. So that was the first time I saw him and I was very impressed.

MB He'd been to North Africa and done that survey on war wounds with [Hugh] Cairns, which was a tremendous piece of work.

JG Yes, indeed. And he'd been to the States.

MB To set up the industry effectively, with Heatley.

JG That's right, yes.

MB So you encountered him then in about 1944?

JG Yes.

MB But it was to be about two or three years later that you came to Oxford to work as a student of his?

JG That's right, yes.

MB You were precipitated away, as it were, from clinical medicine.

JG Yes. Medicine had always been something laboratory based for a number of reasons. Although I didn't dislike and I really quite enjoyed the clinical side of medical training, and also I worked on the house for a short time, I really in a positive way always wanted to work in a lab. The Secretary of the Medical Research Council gave me an interview much to my surprise. I wrote to him saying that I would like to do research.

MB Sir Edward Mellanby at that time.

JG Yes, Sir Edward Mellanby. I wrote to him saying that I would like to do research. I had got no ideas and asked if there were any openings. He said, 'Come and see me.' I went and saw him, had a brisk interview at which he said that I wouldn't be any good and there was no money in it and I was crazy and so on. Curiously, I didn't take any offence in 1947 at being talked to like that. I don't think you could get away with it now, but it seemed to be the way people talked to you. I mean, he didn't say it in any aggressive way, you know, he just said in a very matter-of-fact way, 'You won't be any good, Gowans, there's no money in it and you're crazy.' It was all very neutral stuff and I sort of nodded wisely. But he said, 'If you really want to do it, there's Florey in Oxford who is looking for medically qualified recruits to take on in the lab now the penicillin work is winding down. Would you like to go to Oxford?' This was in 1947. Well, this was really good news. So I said, 'Yes, would I! That would be marvellous.' He said, 'Well, Florey, he's the best experimental pathologist in the country, but you won't like him.' I mean, why say that to me? I had never met the man, you know. But, anyway, that's what he said. So I went back. It was the very hot summer of 1947, I remember. I was thinking, well, I may or may not hear, but quite soon I heard. Again rather like the swiftness of the Mellanby reply came the swiftness of the Florey one. I had a little note from Florey: 'Dear Gowans, come and see me in Oxford.' So in that summer I went up to Oxford. I went to the Dunn School and was shown into his office by Miss Poynton, his secretary. He wasn't there immediately so I sat in his office. And I can remember sitting there, very hot and the sun was streaming in. I was feeling very hot and uncomfortable and probably rather nervous too, and then this tough guy walked in. He didn't say anything. He tore off his coat, hung it on the rack, tore off his tie and his very first words to me were, 'It's bloody hot in here.' Those were the first words Florey ever said to me, as if I was an old friend of his. It actually wasn't terribly relaxing. I didn't know quite what tone to adopt to him in reply to that. He said, 'So you want to do research, Mellanby tells me.' And he more or less repeated what Mellanby had said. He said, 'Well, you're crazy Gowans, there's no money in it and you won't be any good.' But I could take this on the chin by now because I was

getting used to it.

MB Standard story.

JG Yes, it seemed to be the standard thing to say, and it bore no relation to success or failure, you know. Anyway, he said, 'That's fine.' Now, an important point to make about Florey's attitude to his work was he regarded experimental pathology as applied physiology. He thought the whole of the sort of work he did was underpinned by a sound knowledge of physiology and biochemistry. I learnt that later. So he asked me, 'Do you know any physiology?', at this interview in Oxford. I said 'Yes, I've done a second MB at King's in the Strand.' And he said, 'Gowans, you don't know any physiology then.' And he sort of growled at me and he said, 'I think you had better go back to school because you're not house-trained.' I was being reduced to a fairly impotent recruit by this time, what with Mellanby and Florey, and now I wasn't house-trained. Now, I was beginning to learn the style. Being not house-trained meant that I hadn't worked in a lab, I didn't know my way around the lab manipulatively and so on. It was a fairly legitimate comment because I didn't. So he sent me back. I came up to Oxford at the start of the autumn term in October 1947 and I went back and joined in with the undergraduate course in physiology, the honours school of physiology and the final year of it, the third year of it, for one year doing physiology and biochemistry.

MB In Sherrington's old department?

JG Yes, which itself was a link with Florey, you see.

MB They worked together in the Twenties?

JG Yes, you know that story, that's something from Florey's past. Sherrington long since dead, of course. Anyway, that was a very good idea and I think it's worth saying here - I've recounted this story in a rather jokey way because that's how Florey was and that's how he appeared to me with these one-liners, you know, and this sort of tough thin - but underneath his treatment of me was a very practical, down-to-earth, not advice but a bit of shaping my career. He was right, I mean I wasn't house-trained; I hadn't worked in a lab. He couldn't put me in a lab and say get on with it, I didn't know my way around. I'd never worked in a lab as a wartime medical student. Secondly, it was really very valuable doing that year with physiology and biochemistry. I had tutorials with very good scientists. I learnt how to talk as equals to good scientists in tutorial situations. I used a library for the first time. I read essays. I read all the great physiology that was going at that time. I read Hodgkin and Huxley papers, which were just coming out, and all about the Oxford School of Respiratory Physiology and so on.

MB So the groundwork year.

JG But it also set standards, benchmarks for the classic bits of science in the areas that might be of interest, so I'm very grateful to Florey for having given me that sort of start.

MB Did he cast you adrift for that year and just let you make the grade?

JG Yes.

MB So you didn't see him until you came back?

JG No. I think I probably never saw him. I think he enquired about me through the tutors. At the end of that year when I'd finished it, he took me to dinner in Lincoln. He was a professorial fellow of Lincoln College, and there he was clearly a very popular fellow, they liked him there and he mixed well. I remember I got sat next to a don who I think was a bit deaf so he did all the talking. He talked to me about Greek vases for most of the evening and how he had pieced together a lot of fragments of Greek vases into the finished article and how he had learned to identify the artists who depicted the scenes around Greek vases, and there were rather few of them so you could say that this was by X and that was by Y. I was absolutely fascinated. It was Sir Michael Beazley[1] and Beazley is one of the great experts. I didn't realise this. So I began to think that all knowledge was one when I went to Oxford, thanks to Florey, and that there were these other people around studying classical antiquity, which was just like science, you know. You had these problems; you had these formidable techniques and so on. So I saw Florey in this setting in which he was very comfortable. I think he liked college life and, to jump a very big gap, a lot of people, I suppose, were surprised when he became Provost of Queen's College.

MB In the Sixties, in '62.

JG Yes, in his retirement, and they thought that this wasn't his scene. In fact, if you had seen him in College you would have realised that he found it an agreeable sort of society.

MB A kind of family.

JG Yes. He was always criticising the fellows. I can remember him saying, 'You'll never get any research out of the fellows, Gowans. They're too busy teaching.' And I congratulated him when he became Provost of Queen's. 'Oh, it's easy, Gowans,' he said, 'all I have to do is pour drink down the fellows' throats,' which is a fairly primitive view of the duties of the head of the house, I would have thought, but that was his view. But I wasn't surprised. I think he was comfortable in an academic community.

MB And he gave Queen's a lot.

JG Yes, he did.

MB He was a great catalyst.

JG Sure.

MB When you came back from this year of physiology, proving yourself, you

[1] Sir James Gowans must be referring to Sir John Davidson Beazley (1885-1920) Professor of Classical Archaeology, Oxford University, 1925-56.

came back to Florey, back to sit at his feet as it were, and to do PhD studies with him as director of studies.

JG Yes, he was my supervisor. And he gave me a little problem.

MB He set it and he really didn't discuss it, he told you what it should be?

JG Sure. He told me what to do. He didn't say, 'You'll do this.' He put it quite nicely and he said, 'There's this problem and it follows on from my work on antibiotics. There's the question of to what extent is an inflammatory response necessary for an *in vivo* cure of infectious disease when you give antibiotics? I mean, if an agent can really kill bacteria presumably it doesn't matter if you don't have an inflammatory response. We think penicillin is in that category, but we think the tetracyclines need inflammatory response because they are only bacteriostatic. So why don't you find out *in vivo* whether that's true?' So you get experimental animals with and without inflammatory reactions. You have to depress the ability to form an inflammatory exudate in animals and you infect them, and then you treat them respectively with penicillin and the tetracyclines. And you find that the penicillin cures the animals that don't have any inflammatory response and the tetracyclines don't. It's very simple, you see. Very simple. That's a Florey type experiment. If you do it right, it's really like his experiments on penicillin where you have- it's not quite as perfect as this - but in principle you have ten mice alive in one cage and you have ten mice dead in another cage and 'Gowans you don't need any statistics for that experiment'. Now, if I did my little experiment right, it would come out black and white, and that's how he saw science a bit. He didn't like the in-between experiments that weren't clear-cut and he certainly didn't like statistical analysis. Some people regard that as a failing and in certain areas of science it is. But it meant that he did simple clear-cut experiments, which, if they were done properly, gave unequivocal results. And the simplicity of it could be interpreted as a certain lack of analytical powers, if you like, or deep intellectual insight. I don't look at it that way.

MB But it seems that he set up a very simple model of such straightforward questions and said that it's either yes or no.

JG Yes, it can be dignified with the word, it was his *style* of work.

MB And for a research student, that must have been a godsend because you had it all to make, it was going to come out yes or no.

JG Yes. The only problem about an experiment of that kind is that there is not much to talk about.

MB You just get on with it.

JG Exactly. And that too was his hallmark because he wasn't partial to long theoretical discussions and what he used to call 'hot air'. 'You can come to me and talk when you have done the experiment,' you know, 'but don't theorise and so on. I don't want too many conjectures.' There was a sort of analogy. I remember reading about Ernest Rutherford once, who was a New Zealander, and he once said in his lab, which of course was one of the most distinguished physics lab of its era, 'I'm not

going to have anybody talking about the universe in my lab.' And that was rather like Florey. He wasn't going to have anybody talking about the universe in his lab; he wanted to know whether those mice died or not.

MB In a way, he cut you adrift to sink or swim and you came back and told him the answer. Is that right?

JG Yes. I mean, I haven't actually said that yet. What I've said is very simple: he gave a good set of experiments to do and left you to get on with it, and don't talk too much, I mean, just get on. In fact, it was a little bit like trial by water. You were thrown in and if you could swim, that's fine, and if you couldn't, that's really too bad. We all got a little bit depressed. I can remember walking around Oxford with one of the other DPhil students who had just been told it wasn't going very well by Florey. He was terribly depressed and we tried to cheer one another up. The problem was, I suppose, partly that he was never very enthusiastic about the work and talking about how wonderfully interesting it all was, and new ideas in science which had excited him. It was a very practical down-to-earth business. So the day-to-day currency was to be asked when you met Florey in the corridor whether you were still going backwards. 'Are you still going backwards, Gowans?' I used to say, 'Yes, Sir. I mean, No, Sir.' Not knowing quite how to reply. But this was a standard thing. It became a joke in the lab. But it was Florey insulating himself against perhaps an intimacy with his collaborators, which he didn't wish to develop. I mean, he was a man who one didn't really get to know. One admired him enormously, partly because of his achievements. He was a great man by that time with all the right accolades, but he also had an authority and a drive, which were irresistible, in the lab, I thought. The only point at which one later had doubts, in retrospect, not real doubts because nobody has everything, was that you couldn't call him an intellectual virtuoso, you know, profoundly changing the course of human knowledge by an idea where you can say that biology before Florey looked like this and after Florey looked different, as you could say for Sherrington or others. He wasn't like that. But he was very taciturn and a little bit unapproachable, so I suppose we were a bit frightened of him because of this toughness.

MB This great private man.

JG Yes. I mean, in the thirty years that I knew him - he died in 1968 - he never called me by my Christian name, ever. It was always Gowans, but it was the same with everybody in the lab. I never heard him call anybody by their Christian name. I'm sure he did with his friends, but his colleagues in the lab were always Heatley and Abraham, you know, even the very old ones.

MB So I am getting a picture of a remote, but in a way, privately caring supervisor.

JG Yes, I think that's right. I think he did care.

MB I am thinking that while you were doing PhD studies, you told me at one time James that you were laid up with illness for a time.

JG Yes.

MB And I think he showed caring at that time?

JG Yes, he was good there. This was in the middle of my PhD. I'd done one year's PhD and I got a tuberculous pleural effusion and I was away for a year, as one was in those days. I was in the Churchill [Hospital, Oxford] at first stuck in a bed, and he came up to see me and said how sorry he was. He had been in touch with the MRC about my studentship, which would go on for another year.

MB You were going to be all right.

JG Yes, for a year, it wasn't going on forever. But he'd done all that with Mellanby and checked it out. So it was very nice for him to come up, but he was quite embarrassed. He didn't sit on the bed and tell me jokes. He didn't know quite what to do. It wasn't his scene. But he'd done what was necessary, so I'm sure he did care, yes. But he found it hard to show it, you know, to compose himself.

MB When you get into the 1950s with your DPhil you then decided to stay on and to work further with Florey with the Dunn School kind of set-up. That work slightly changed at that stage and you moved away from the antibiotic arena. I think he set you another question.

JG Yes. I went to Paris. After my DPhil, I had a studentship at the Pasteur Institute in Paris for a year and then came back to the Dunn School. And he said, 'Well, what do you want to do?' I said I wanted to do immunology because I'd learnt a bit about infectious diseases and immunology from working in Paris, in Grabar's lab, and meeting immunologists there, and some very good scientists were there at the time, you know. There was François Jacob and [André] Lwoff and [Jacques] Monod and so on. They were all in their log phase there. So I said I wanted to do immunology and he was absolutely appalled because, you see, that's not the reply you give to Florey. What you say to Florey is what you are going to do when you come in the lab tomorrow. That's what I hadn't said because I hadn't the slightest idea what I was going to do when I came in. I just wanted to do immunology. He didn't think much of that.

MB You'd not thought it through.

JG No. I just wanted him to say that immunology was wonderful, you see, which was what a lot of supervisors said, but not Florey. He thought this was terrible. So he said all right. Then he described a problem to me which he said he thought I might be interested in, and this was my start in my scientific career, so to me it's been incredibly important because he put to me the lymphocyte problem. And he described it very clearly, he said, 'Lots of lymphocytes go into the blood every day and you can tell that by cannulating the major lymphatic ducts, which are full of lymphocytes, and we've done that in the lab, we've been cannulating ducts here for a long time. The numbers in the blood don't rise, so numbers must leave must leave the blood every day in equal numbers. Where do they go? Simple. You find out.' Well, that's a very cut and dry problem. X go in, numbers stay constant, X go out. Where does X go?

MB A black box experiment. Typical Florey model?

JG Yes, and explained terribly simply, you see. But it was a key question because the question of the fate of these disappearing lymphocytes had given rise to a lot of speculation because the function of lymphocytes was unknown. They were the mystery cells of experimental pathologists, always turning up in interesting situations but nobody knew what they were doing, and here they were in huge numbers entering the blood and disappearing. 'If you can find out where they go, Gowans, you can find out what they do.' Which was reasonable at that time. There were theories about it, the major one being that they went to the bone marrow and became precursors of other blood cells.

MB That was the popular view, I think.

JG That was the major view at that time, yes. Anyway, Florey wasn't interested in that. He said, 'Just find out where they go so we know where they go and then we can think about what they do.' Now, there was a tradition of work on lymphoid tissue in the lab. Sanders in the lab had done work with Florey on lymphoid tissue and Florey had adopted the very reasonable view of physiologists, that if you want to know the function of an organ, excise it and observe the consequences, you see. So he said to Sanders, 'Cut out all the lymphoid tissue in the animal, all the Peyer's patches and the thymus as well as the lymph nodes, and see what happens to the animal.' Well, of course, all the animals died because they died of perforated guts and things. I mean, it was a heroic thing. Anyway, Sanders did this and what he found was that little bits of residual lymphoid tissue regenerated. You couldn't cut it all out, the bits that were left behind just regenerated and so on, so it didn't come to much. They'd cannulated some lymphatics and observed the turnover problem, which I have described. So Florey summarised it by saying that 'the lymphocyte problem has blunted the whips of a lot of people in the lab, Gowans, and I don't see why you should be spared a similar fate,' which is another piece of 'Floreyana', you see. By that time I expected him to put it that way, but he described the problem very clearly. He also led me to a paper that had been published a year or two before describing the way of cannulating the thoracic duct in a rat and collecting lymphocytes from a non-anaesthetised animal, that was the technique. So once I had to learn how to do that and then I was launched. I had to figure out how I was going to do it all. He didn't suggest labelling the cells and he didn't suggest re-transfusing them. He said, 'Just learn how to cannulate them and think about it.' But the problem was very clear-cut and I owe it totally to Florey for the launch pad.

MB Which led eventually to the real discovery of the purpose of the B-lymphocyte.

JG To the general function of lymphocytes, that they underwrite immune reactions. The T and B stuff was Jacques Miller in Australia. I knew Jacques in England while he was working here. Florey followed the work very benevolently. I remember he came in one day when I had all this apparatus re-infusing lymphocytes into the bloodstream, collecting them from the duct, and he actually said, 'Very good, Gowans, very good.' He had never said that to me before.

MB That was a bit over the top for Florey.

JG Yes, and I was really taken aback at that.

MB Because I'm recollecting a time, you did tell me, and it's a wonderful story, about the day you got a fellowship in College and he came close to a congratulatory kind of greeting.

JG Yes, that was the sort of classical Florey. I got a fellowship at Exeter College called the Staines Medical Research Fellowship, which was a minor success, but it gave me a salary, you see, and somewhere to live because this was before I was married. Florey came into the lab, to room 45 in the Dunn School, to congratulate me and he put his head round the door and congratulated me and I said, 'Thanks very much.' Except that he didn't say that, you see. What he said was, 'I hear you got the Staines Fellowship. You were lucky, Gowans, it was a poor field.' And do you know, I said thank-you. I was delighted because I know what he meant to say was 'congratulations', but he couldn't say that. He said, 'You were lucky, Gowans, it was a poor field.' And he was probably right, you see, it was probably a terrible field. But I thought that was a remark verging on genius because I couldn't have thought of that, nobody else could have thought of that remark. But it was just that he couldn't bring himself to say congratulations. That was what he meant, you see, because it was meant in good faith. He was masterly in that way.

MB James, what I am trying to encapsulate in these next few minutes is the way in which he helped you and assisted you through those 1950s, which were packed with research, packed with investigations. You were working largely alone in a laboratory, virtually alone, with Henry Harris sometimes around, but you were working rather solitarily. He was a good support generally I'm trying to say

JG Yes, he was. I've mentioned room 45 and I worked on one side of a bench and Henry Harris worked the other side, and we shared the room together until much later we got a unit, and that's really post-Florey time, the unit. But, yes, he was supportive. He understood the re-circulation story and he was very supportive of that because it was a very clear-cut set of experiments. He understood all that. But then we got on to the immunology, and that was interesting because I got on to the immunology, I suppose, largely through a friendship with Peter Medawar, when I asked his advice about whether to try and make animals tolerant in experiments in which animals receive transfusions of lymphocytes rather than the animal receiving its own back. And of course the transfusion of lymphocytes in other animals are tissue transplants, and so I was asking Peter whether to make animals tolerant. He said, 'That's a waste of time. Why don't you make inbred strains?' That was the advice I got. But I got to know Peter. He became very interested in our work, and through fag-ending his stuff on transplantation immunity we eventually showed that the small lymphocyte, this mystery cell that Florey had put me on to, could cause graft against host reactions, which I learned from Medawar, and so on. So it was through Peter Medawar that I got an entrée into immunology. The interest there was, of course, that Medawar was an old alumnus of the Dunn School. Peter Medawar had done his earliest research in the Dunn School and Jean Medawar, his wife, had also worked in the Dunn School and Jean Medawar had published a paper on lymphocytes about lymphocyte motility. She had an early grievance, which was never resolved, that everybody wrote to Peter for the reprints because in the list of references it came 'Medawar on lymphocytes'. Everybody thought it was Peter and it was Jean, actually. Anyway, that was nice link between my friendship with Medawar. Now,

Medawar and Jean used to come to the lab and Florey used to unburden himself to Jean Medawar. He would talk very freely to her - I felt that - and he and Peter Medawar got on like wildfire. Peter thought that Florey was just about the funniest person he'd ever met. If you ever saw them together, there was always Peter collapsed in helpless laughter, genuinely, because Florey's rather gravelly style, all these one-liners, you see, meant he could be genuinely very funny. And he had an enormous fund of knowledge about things. I mean, he wasn't a narrow man at all. So he could be a very interesting companion and he got on very well with Peter. I have pictures of them collapsed in laughter, mutually, you know, between them. They got on very well. So for me, who'd gone to sit in a sense at Medawar's feet to learn about immunology, it was all in the family because Medawar in turn had worked with Florey. But, yes, he was supportive without being exuberant or demonstrative. I should say that in later years, before he became President of the Royal Society, I don't know what happened, but he became more and more reclusive in the lab. I was more or less self-propelled by that time. I had my own little problem so it didn't hinder my activities. But he had a sort of set of traffic lights put up outside his door, red, amber and green, and when there was a knock on the door he used to press a button inside which signalled to those outside what to do, and it usually went red, sometimes amber, which was very distressing because it meant that you just had to wait outside the door, you see. But it was a device to insulate him from whatever it was he wanted to be insulated from. I never figured out those days. The cloud lifted when he became President of the Royal Society.

MB Yes, it all changed and he became a new man, so I'm told.

JG Well, that was my reading of it because he went to the Royal Society to be President; he told me much to his surprise. I mean, I wrote to him and congratulated him and I think he didn't reply, but I saw him in the lab and he said how surprised he was; something like that he wasn't clever like the other ones, you know, and it was a great surprise. He turned out to be an extraordinarily effective and successful President of the Royal Society on two grounds. One is that he was an innovator there. He, as you know, transferred the rooms of the Royal Society from Burlington House in Piccadilly into Carlton House Terrace. He fixed up the move and the building and all the rest of it, and that was a major undertaking, and he introduced lectures into the Royal Society, a series of lectures. He made it a centre for discussion meetings and symposia, so the place became alive because it had been rather dead beforehand, I think. In government he was very effective. I'd seen him with Hailsham, who was the minister of science, at a public meeting and they got on very well together. You see, Florey was somebody. He had this tremendous card to play about penicillin, which was an entrée into any door, political, scientific, social. That was 'the man that pioneered penicillin', you see, that was a great card to play. But he also had, as I have said before, an authority and a power behind him, you could see that. He was a powerful man. You don't cross him, you know, you just get to know him, and he was full of common sense. It was almost common sense carried to the point of genius. I mean, he could sort of figure out how to do things and it all sounded so simple when Florey had a solution.

MB He reduced everything to basics.

JG 'Why don't we just move to Carlton House Terrace?' you know. And, 'Why

don't we just put up a building for Queen's students to live in?' - the Florey Building, you see. 'Why don't we write a great textbook of experimental pathology in which you all contribute to?' and in its day that textbook of general pathology, which Florey edited and was entirely his idea and most of his work, was the best book in the world on general pathology and when he died it died with him.[2] He took on big projects and he was a very hard worker. But, as I say, he came into his own when he was President of the Royal [Society] and I know that because I used to meet him there. I heard him give speeches at Royal dinners and they were all very affable and he seemed very much at ease. But the office staff adored him. I used to meet the secretaries down there; there was a girl, who looked after me because I was a research professor, who thought Florey was absolutely wonderful.

MB A wonderful fatherly figure at that time.

JG Well, that and charming and attentive and good fun and everything. All the nicest things you could say about a person. So, obviously, he found his feet there or he just found something which was extraordinarily agreeable and convivial for him, so he was very effective there.

MB James, we're coming to the end of this particular session together, but it would be nice to move to a final appraisal of Florey in your life because he had this profound effect on the development of your career. You've said already that it never got as far as, despite the years and the working closely together, as far as first name terms but you were close.

JG Yes. I mean, I've emphasised that I owe my career to him and it was through his generosity that I landed jobs and was supported all those years and had space in the lab. So he was my father figure; he was one of the most important people in my life. In that sense, he was a person of great importance to me, but he wasn't close in the sense that a friend would be close, a close friend. I mean, one never took personal problems to him, domestic problems or worries; it was a distant closeness if you see what I mean. It was a very professional relationship, but that doesn't prevent the relationship from being very important and making me acknowledge, which I'm very happy to acknowledge, that he was a very formative and important influence in my life. I think he was a great man.

[2] Florey, H.W., ed., 1954. *Lectures on General Pathology: delivered at the Sir William Dunn School of Pathology, University of Oxford.* London: Lloyd-Luke (Medical Books). Subsequent editions entitled: General Pathology.